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October 5, 1998

Mr.John Vierow
MS R-2-3 SAIC
11251 Roger Bacon Drive
Reston VA 20190

Dear Mr.Vierow,

Attached please find a copy of my report reviewing the document *Estimating the risk from the disposal of solvent-contaminated shop towels and wipes in municipal landfills* by Research Triangle Institute. I have tried to be responsive to the questions posed in your reviewer's instructions, and to indicate the means by which my recommendations could be followed. I have also enclosed a copy of an invoice for my services.

I mailed back the contract information Friday after I received it from SAIC.

Sincerely,

William A. Jury
Professor of Soil Physics

Estimating the Risk from the Disposal of Solvent-Contaminated Shop Towels and Wipes in Municipal Landfills

Reviewer Comments

My review first addresses issues as they are encountered in a review of the sections of the document I was asked to evaluate. Next, specific questions posed to the reviewers are answered.

Chapter 2

p. 2-1 You state that the assumptions made in describing the contents of a spent wiper were based on data collected in the economic analysis. I wonder about the statement that the solvent is not bound to the wiper matrix, which essentially liberates all of the solvent to the landfill upon disposal. This is a very conservative assumption.

Chapter 3

p.3-3 Table 3-1 Obviously, you use the same volatilization rate constant for each site. In effect, this means that you are assuming each site is at the same temperature and has the same windspeed characteristics.

p. 3-5 and 3-6 There is an alternative to just using loam as the standard soil. A lot of work has been done in the last decade on producing characteristic properties for the principal soil types. Van Genuchten's group at the US Salinity Lab in Riverside has generated data and parametric representations for the hydraulic and retention properties of the 7 standard soil types.

Chapter 4

p. 4-9 I am uncomfortable with your handling of degradation. Much of the reported literature on degradation is from lab studies where degradation was specifically isolated. When data is reported from field studies, it not only includes contributions from the other pathways as you state, but also a substantial contribution from sampling error and sampling inadequacy due to spatial variability. Since you have adopted a statistical approach, you could have used a range of values for each compound. Or,

as an alternative, you could have placed the compounds in groups of high, medium, low, etc. persistence and used representative values for degradation. Looking at the values in Table 4-5, the compounds are all assigned high persistence, but my experience suggests otherwise. The book by Howard is referenced as the source. However, it doesn't appear to be used the way the authors suggest. For example, methanol is assigned a half life of 36 yr. However, Howard says on p. 311 of volume 2: *Methanol is expected to be significantly biodegradable in soil based on the results of a large number of biological screening studies.* He quotes a number of studies on the next page.

p. 4-11 I understand the reasoning behind developing a shortcut to the enormous computational load required to calculate groundwater concentrations, but the DAF is fraught with problems (see below).

Appendix A

p. A-2 Your Henry's constant conversion is correct only if the solubility is given in moles per m^3

p. A-3 The flux of water vapor does not enhance transport to the soil surface. It is the upward advection of dissolved contaminant with the liquid water flux that enhances it. The evaporation process causes the surface concentration to increase.

p. A-4 There appears to be confusion over what was done in the Jury papers to cover the case of simultaneous upward flow of water and diffusion of chemical. This problem was solved in Jury et al. 1983, and was presented as Eq. 24 of that paper (which had an error that was reported a few years later as an erratum). The decomposition in Jury et al 1984 was done to evaluate the role of the boundary layer. Thus, the elaborate procedure of using part of the Jury equation together with an equation that describes diffusive transport only produces error. The model described in the report also uses both downward flow and upward flow at various times, restarting the equation because of the well-mixed layer assumption. This method of calculation is difficult to assess without using a numerical model as the standard. I understand that you want to use the simplicity of the analytic solution, but that you have dynamic upward and downward flow events. However, the way you do it can produce a lot of error in certain circumstances. For example, imagine a case where a volatile and mobile chemical was subjected to a period of leaching, followed by evaporation. The real physical case would not have that much volatilization occurring for a while during the evaporation period, because the chemical would be leached into the subsurface and have a soil layer above it at the beginning of the evaporation stage. However, the way you calculate it, volatilization would begin with a high (uniform soil). I don't

think the net effect of these two processes calculated separately would be the same as if they were together. I have no idea what you mean by having small time steps make the two solutions coincide. That would not make the error of separating the physical processes any different in my opinion. I think that this needs to be investigated using numerics as the standard. I see no other way to assess the potential error involved in this artificial decoupling. I believe that Scott Yates at the US Salinity Lab in Riverside has a numerical version of the Jury model that accepts transient inputs.

p. A-4 and A-5 none of these equations include degradation, which is part of the Jury model. Again, it appears that an artificial decoupling has been used wherein degradation is calculated separately. This also should be checked with a separate calculation.

Appendix D

The DAF is developed by using a numerical model as the standard and doing a number of calculations with a range of parameter values to establish probability bounds for values. Although this makes me uneasy, it has the virtue of being general and is being used in a screening sense to rank compounds. My major sense of unease comes when I see hazard indices put in boldface as they are for Pyradine and a few other compounds in various scenarios. This implies that some scientifically-based health standard has been exceeded, which cannot be concluded when this many assumptions and simplifications have been made. The DAF only dilutes by dispersion. There is no degradation operating in the generation of the values. The DAF also is generated assuming advective-dispersive transport, which is not true near the point of entry of the contaminants in ground water.

p. E-25 Table 1 A serious error appears to have been made in selecting dispersivity values. There have been only three well-instrumented ground water transport experiments conducted at a sufficiently high level of resolution to measure dispersivities, and they are much, much smaller than yours. Borden (Mackay et al. 1986) and Cape Cod (LeBlanc et al. 1991) have longitudinal dispersivities of 0.43 and 0.96 m, respectively, and Missouri (Boggs et al. 1992) which is generally regarded as an extremely (some say pathologically) variable aquifer, had a dispersivity of 12.8 m, which is about the size of your median value. You appear to have selected values from regional scale analyses, which are inappropriate to use at the short distances. For example, the ratio of your dispersivity to your observation distance (25 ft) is less than one in some of your scenarios, which is unphysical. Observations of dispersivity follow what is called the scale effect, with the apparent value growing with distance from the source. Gelhar (1992) discusses this in his review article and summarizes the

world data base. Also, the ground water velocities you use are atypically large, and there is generally an inverse correlation between conductivity and hydraulic gradient. Thus, you would never see a gradient of 0.05 with a high K_s value such as you have in your High scenario.

Mackay, Douglas M., et al. "A natural gradient experiment on solute transport in a sand aquifer; 1, Approach and overview of plume movement." Dec. 1986. (WATER RESOURCES RESEARCH ; Vol. 22, No. 13, p. 2017-2029)

LeBlanc, Denis R., et al. "Large-scale natural gradient tracer test in sand and gravel, Cape Cod, Massachusetts; 1, Experimental design and observed tracer movement." May 1991. (WATER RESOURCES RESEARCH ; Vol. 27, No. 5, p. 895-910)

Boggs, J. Mark, et al. "Field study of dispersion in a heterogeneous aquifer; 1, Overview and site description." Dec. 1992. (WATER RESOURCES RESEARCH ; Vol. 28, No. 12, p. 3281-3291)

15) Gelhar, Lynn W., et al. "A critical review of data on field-scale dispersion in aquifers." July 1992. (WATER RESOURCES RESEARCH ; Vol. 28, No. 7, p. 1955-1974)

Questions to Reviewers

A. Overall Risk Assessment

1. I believe that in its present form the DAF may produce values that have little meaning for decision-making purposes. However, the problem appears to be the use of unrealistic values for the scenarios by the EPACMTP model that generated the DAF, rather than the DAF concept itself, which may be adequate for relative screening of compounds, but not absolute decisions based on exceeding compliance levels. My recommendation is to redo the calculations with more realistic parameters for dispersion and velocity.

2. The factor which would tend to underestimate risk is the DAFs generated from the large values of dispersivity. Some of the other factors, like degradation rates, appear to be conservative.

3. The factors associated with transport should scale if the solute transport and reaction processes are linear, not a bad assumption. I don't know whether exposure can be scaled.

4. Assumptions are discussed in the specific comments section.

5. Although it is mentioned, I believe that the role of process uncertainty should be more strongly emphasized. The particular type of uncertainty analysis performed here assumes that the equations are exact. Thus, overall uncertainty is underestimated.

B. Partitioning Methodology

1. The description was adequate for me to understand what was being done. Some errors were noted in the specific comments section of the report. I am sympathetic to the motivations that led to the use of the particular type of compartmental model for the landfill, and aspects of it were applied cleverly to handle the scenario of covering waste with clean soil. But the authors ventured into murky territory when they took a simple analytic solution designed to represent an idealized situation (uniform initial condition and uniform unidirectional water flow) and used it to represent both upward and downward flow events more or less simultaneously. It also uses a uniform well-mixed compartmental concentration to model processes that depend strongly on concentration gradient, and arbitrarily decides to use existing data on degradation to represent total loss rate from all mechanisms. As a result of all these rather unique manipulations, I have no idea how well the model works. That is for them to decide, by comparing its performance to a numerical representation of the true system they are approximating. I suggest where they might look for such a model in my comments above. Their approach forces mass balance, but may not have mass leaving the system via the appropriate routes at the appropriate rates.

2. I am not confident that laboratory leachate data would be a superior approach to estimating leachate losses. There is a huge difference between the lab and the field and the translation from one regime to the other is an ongoing research problem. As to how well the partitioning equations work I cannot say. Testing them against a numerical code would provide some insight, but even the numerical approach would be forced to use assumptions that neglected spatial variability and other factors that we don't have a good way of modeling yet (eg preferential flow).

3. I have addressed what I could of this question in the specific comments section above.

D. Ground Water Transport and Modeling

1. Since the DAF is used to represent the effect of dispersion on dilution of concentration from source to receptor, your question boils down to two issues: how reasonable is it to assume that all compounds are mixed the same by the processes of small-scale advection, and how reasonable is it to assume that all compounds are equally persistent in ground water? Since the K_{ow} values vary by almost four orders of magnitude, and since at least some degradation would occur in the subsurface, it is certainly reasonable to expect that neglecting the effects of adsorption and degradation on dilution is a potentially large source of error. However, ground water degradation rates are almost nonexistent for the compounds studied, and in the absence of degradation, I believe that adsorption would scale out of the steady state form of the 3-D plume solution. Thus, it is not clear how to improve the DAF if it is used.

2. The representation of the individual constituents is about equally good (or bad) at the screening level. The burden falls entirely on how well the chemodynamic properties have been selected and as noted above, I have concerns about degradation.

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Education

- B.S. Physics, 1968, University of Michigan Ann Arbor
- M.S. Physics, 1970, University of Wisconsin Madison
- PhD. Physics, 1973, University of Wisconsin Madison

Professional Experience 1988-present

- Professor of Soil Physics, Department of Soil and Environmental Sciences, University of California, Riverside, 1982-present
- Interim Chair, Graduate Program in Environmental Toxicology, University of California, Riverside, 1987-1988
- Chair, Department of Soil and Environmental Sciences, University of California, Riverside, 1990-95
- Director, Graduate Research Unit in Environmental Science and Engineering, University of California, Riverside, 1997-present

Professional Honors and Awards

- UC Riverside Graduate Student Association Distinguished Teaching Award, 1981
- UC Riverside Faculty Academic Senate Distinguished Teaching Award, 1986
- Fellow, Soil Science Society of America, 1988
- Soil Science Research Award of the Soil Science Society of America, 1989
- Guest Professor, Swiss Federal Institute of Technology, Zürich, 1989
- Dupont Research Lecturer, University of Delaware, 1991
- Environmental Quality Research Award of the Agronomy Society of America, 1992
- Fellow, American Association for the Advancement of Science, 1995
- Fellow, American Geophysical Union, 1996
- Appointment, Water Science and Technology Board, National Research Council, 1997-2000

Research

Research Interests and Achievements

Dr. Jury's principal research interests have been in the areas of measurement and modeling of organic and inorganic chemical movement and reactions in field soils; development and testing of organic chemical screening models; characterization of the spatial variability of soil physical and chemical properties; and assessing volatilization losses of organic compounds. At present, he is conducting research on dissolution of nonaqueous phase liquids in soil; gas movement in structured soils; field measurement and modeling of preferential flow of tracers, pesticides, and viruses; degradation of pestidices and VOCs during transport; measurement and modeling of Selenium fate in soiland degradation during transport of volatile organic compounds. He has published 196 professional papers and written four books in his areas of research.

Representative Peer-Reviewed Publications (from a total of 196 and four books)

Jury, W. A.
1982. Simulation of solute transport using a transfer function model. Water Resour. Res. 18:363-368.

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1989 Field scale transport of bromide in an unsaturated soil. II. Dispersion modeling. Water Resources Res. 25:1582-1589.

Ghodrati, Masoud and W. A. Jury, 1990. A field study using dyes to characterize preferential flow of water. Soil Sci. Soc Amer. J. 54: 1558-1563

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Jury, W. A., J. S. Dyson, and G. L. Butters, 1990. A transfer function model of field scale solute transport under transient water flow. Soil Sci. Soc. Amer. J. 54:327-332.

Utermann, J. E. J. Kladivko, and W. A. Jury
1990 Evaluation of pesticide migration in tile-drained soils with a transfer function model. J. Env. Qual. 19: 707-714.

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1990. *Transfer Functions and Solute Transport Through Soil: Theory and Applications.* Birkhaeuser Publ. Basel. 235 p.

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1991. *Soil Physics.* John Wiley & Sons New York 328 p.

Ellsworth, T. J. W A Jury, F E Ernst and P. J. Shouse, 1991. Three dimensional field study of solute transport through unsaturated, layered porous media 1. Methodology, mass recovery and mean transport. 2. Dispersion modeling. Water Resour. Res. 27: 951-982.

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1991. Transport of chloride through an unsaturated field soil. Water Resour. Res.27:2533-2541

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